Asserted but Unproven: A Further Response to the Lindgren Study's Claim that the American Bar Association's Ratings of Judicial Nominees Are Biased

Michael J. Saks & Neil Vidmar *

All of our data is grossly inaccurate . . . but I need data in order to manage. If I concentrate hard enough, I can forget that the data is bad. Then I can use it.[1]

In this article we comment on James Lindgren's defense[2] of his study of ABA evaluations of prospective federal judicial nominees,[3] which we critiqued in an earlier issue of this journal.[4] In his original study, Professor Lindgren examined the ABA's ratings of sitting federal appellate judges who had been appointed by Presidents Bush (the elder) and Clinton. Professor Lindgren found that, among a subset of these judges, the Clinton nominees received higher ratings from the ABA, a difference that he was unable to account for using certain control variables. Professor Lindgren argued that the unexplained differences were the result of bias on the part of the ABA Standing Committee on Federal Judiciary, favoring Democrats. In our critique, we showed why Professor Lindgren's data provided feeble support for his conclusions. Professor Lindgren's rejoinder sought to defend his original conclusions. We now respond to that defense. We proceed in

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[1] Pointy haired boss to Dilbert. Scott Adams, *Dilbert*, 9/10/01.

[2] James Lindgren, *Saks and Vidmar: A Litigation Approach to Social Science*, 17 J.L. & POL. 255 (2001) [hereinafter Lindgren Rejoinder].

[3] James Lindgren, *Examining the American Bar Association's Ratings of Nominees to the U.S. Courts of Appeals for Political Bias, 1989–2000*, 17 J.L. & POL. 1 (2001) [hereinafter Lindgren Study].

[4] Michael J. Saks & Neil Vidmar, *A Flawed Search for Bias in the American Bar Association's Ratings of Prospective Judicial Nominees: A Critique of the Lindgren Study*, 17 J.L. & POL. 219 (2001) [hereinafter Saks & Vidmar Critique].

the following way.[5] Part I focuses on the central issues of the study's merits. It reprises the major methodological concerns we raised, Professor Lindgren's responses to those concerns, and our thoughts about his responses. Part II addresses several more minor or tangential issues raised by Professor Lindgren in his rejoinder to us.

I. PROFESSOR LINDGREN'S RESPONSES TO OUR PRINCIPAL CRITICISMS

A. Given the Substantial Limitations of the Study, the Conclusions Are Overstated

One of the most important problems that we discussed in our critique of Professor Lindgren's study was his failure to state his conclusions with a degree of caution suitable to the limitations of his data. Instead, his conclusions were trumpeted far more loudly and widely than justified by the study's design and its data.[6] Tempering the

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[5] At this point it may be necessary to say something about the tone of Professor Lindgren's response to our criticism. Professor Lindgren says that in our review of his study we "committed" "errors . . . with surprising frequency," that our errors are "many," "outright," and "obvious;" that we were "laboring under [one or another] misimpression;" that we are "confused," "unaware of [one or another] distinction," and "dense." If that were not bad enough, we are prone to "fabrications," "falsehoods," "false statements," and "disturbingly false" statements. Indeed, it appears that virtually every point in our entire commentary was not only unsound, but was the product either of stupidity, dishonesty or unrelieved incompetence. Of the one table in our article, Professor Lindgren says: "I have never seen a chart in a law journal so badly constructed." Imagine: the very worst table in the history of law journal publishing and it was ours—so far as Professor Lindgren can determine. In contrast, Professor Lindgren observes that he himself "was able to avoid any . . . false statements or outright errors . . . ." and that when he made a choice it was "precisely the right one" (though we still think it was the wrong one and we will explain why).

It does not take much wit to say, not that you disagree with a person and think he is mistaken, but that he is a stupid liar. In our original critique, it would have taken little effort to deliver such treatment to Professor Lindgren. But *ad hominem* attacks are, in our view, inappropriate and unproductive. We prefer to discuss Professor Lindgren's study, not trade insults with him. In our original critique of his article we focused our comments on the merits of his study. Similarly, in this reply to his rejoinder, we will concentrate on the merits of his study and on his defense of the study.

[6] Lindgren Study, *supra* note 3, asserted "strong evidence of differential treatment," at 3, "strong political bias," at 26, "large political differences in the outcomes of its evaluative processes," at 28, and recommended that "the Senate, the White House, and the press" "ignore entirely the ABA ratings because of probable political bias," at 27. Moreover, from James Lindgren, *Yes, the ABA Rankings on Judicial Nominees Are Biased*, WALL ST. J., August 6, 2001, at A13: ABA "not [objective] anymore," "large political differences in [the ABA's] evaluative processes," "Now that there are hard data that support the claims of its critics, it would be good to see fewer denials. . . ."

study's conclusions with an appreciation of the study's limitations would have led not to a conclusion of "strong political bias favoring" Democrats but one of preliminary, partial, and tentative evidence of differential treatment of prospective nominees, suggesting the need for more and better research to confirm the seeming pattern.[7] Indeed, many researchers producing the same data would have regarded them as a pilot study, not yet ready for publication, and many social science journals would not have published the paper without additional and better data.[8]

The nearest to a defense that Professor Lindgren offers is to say that he had acknowledged one or two of the limitations in his paper.[9] But he seems to believe that once you acknowledge limitations you then are free to ignore them, rather than taking them into account when you draw conclusions. In his rejoinder he again ignores the implications of the limitations and reasserts his previous conclusion with even greater recklessness.[10] In other words, important qualifiers—some acknowledged briefly, others not acknowledged at all—are ignored when the conclusions are given and discussed.

Another defense Professor Lindgren offers is the argument that other researchers have made similar errors, and therefore the limitations of his study ought not to be regarded as limitations,[11] or that commentators are estopped from pointing out the flaws. What is important to note at this juncture is that Professor Lindgren continues to miss the point. Other researchers quite routinely present their

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[7] Lindgren Study, *supra* note 3, at 26. By contrast, see the quotations referred to in the preceding footnote. Moreover, Professor Lindgren announced his "findings" in an op-ed column before they had even seen the light of scholarly publication.

[8] As reviewers and editors of countless studies submitted for publication to numerous social science journals, both of us would have made essentially the same criticisms in our editorial reports on the paper if Professor Lindgren's study had been submitted to one of those journals as we did in our critique in this journal, and would have recommended that the study either not be published until replicated using better measures or at least that the conclusions be toned down to reflect their preliminary and tentative quality.

[9] "I openly acknowledge that there are important unmeasured variables." Lindgren Rejoinder, *supra* note 2, at 257.

[10] "One doesn't need fancy regression analyses to see the differences; they are in the raw data. ... They are there; they are large; they are statistically significant." Lindgren Rejoinder, *supra* note 2, at 255. To say that they are evident in the raw data without the need to control for confounding variables ignores the sensible logic that seemed to undergird his study in the first place. *See also infra* note 38.

[11] Including one of the present authors. For a more specific response, *see infra* Section II.A.

conclusions with due regard for the limitations of the data on which their conclusions were based, while Professor Lindgren did not.[12] Professor Lindgren does not so much answer the criticism of overstated conclusions as to repeat his error.

B. The Clearest View of Hypothesized ABA Bias Is to be Found in the Group of Prospective Nominees Reviewed by the ABA, Not in Confirmed Nominees

Professor Lindgren's Study focused exclusively on judges confirmed to seats on the U.S. Circuit Court of Appeals. We argued that the correct target population for a study of whether or not ABA decisions are biased consists of the prospective nominees reviewed by the ABA.[13] We elaborated that this is so not only because it is obvious—if you want to study the population the ABA evaluates, it is best to study the population the ABA evaluates—but because the groups that are created by subsequent cuts can possess relationships among variables that are quite different from those that might be found in the target population.[14] In other words, whatever Professor Lindgren found to be true of confirmed nominees may not be true of the pool of prospective nominees because the pool was reshaped when it moved beyond the ABA. The further downstream in the process the sample is drawn, the more uncertain is the inference back to the target population. The study need not be fatally flawed merely because the sample is not representative of the target population, but it does mean that due caution is required when trying to infer conclusions from what is, really, an analysis of the wrong population. We also argued, and still say, that given the research question that seemed to be stated in Professor Lindgren's study,[15] it also would be informative to see how the ABA treated prospective nominees for district court seats.

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[12] *See, e.g.*, the discussion of the Emory article on juries, *infra* Section II.A.

[13] *See* Saks & Vidmar Critique, *supra* note 4, at 223-26.

[14] *Id.*

[15] Though the Lindgren Study does not state its hypothesis explicitly, it appears to be a test of the general allegation that the ABA is biased in its evaluation of judges. *See* Lindgren Study, *supra* note 3, at Introduction, Section III: Policy Implications, and Section IV: Conclusion. The Rejoinder insists that the precise "hypothesis is stated repeatedly throughout the paper," but the material offered in support of that insistence is, instead, a statement of the study's asserted findings. Professor Lindgren seems to be mistaking his results for his hypothesis. *See* Lindgren Rejoinder, *supra* note 2, at 267-68. *See also* Lindgren Rejoinder, *supra* note 2, at 268 ("Obviously, my conclusion is that any evidence of bias is concentrated among those without judicial experience. Showing that there is no evidence of bias in rating those with prior judicial experience could not

Professor Lindgren's rejoinder essentially ignores this somewhat complex methodological issue by converting it into a question of sample size.[16] In addition, he says repeatedly (and incorrectly) that we insisted that the sample needed to include the unconfirmed nominees.[17] What we actually said, and said in considerable detail, was both more and less than that.[18] Including the unconfirmed nominees improves the sample's approximation of the target population, but still misses it by a considerable distance. By enlarging the sample temporally and adding unconfirmed nominees, as he does in his rejoinder, his defense largely misses the actual point of the criticism. Adding unconfirmed nominees is a step in the right direction, but still falls far short of the correct target population—the total pool of prospective nominees (those who eventually were nominated and those who were not) rated by the ABA. Professor Lindgren's other defense, as mentioned above, is that others have done the same sort of thing, so there can be nothing wrong with the practice.[19] First of all, the logic of whether a given population will serve adequately to answer a study's research question depends on what the research question is and what inferences one tries to draw from the data—that is, whether the conclusions drawn are reasonable in light of the limitations of the population or the sample.[20] Second, the proper

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possibly refute *the actual hypothesis of my paper* because that is part of the hypothesis of the paper—as anyone can plainly see" (emphasis added)).

[16] Lindgren Rejoinder, *supra* note 2, at 265-66 (emphasizing enlargement of the sample size while ignoring the criticism that actually had been made).

[17] "From reading Saks's and Vidmar's essay, one would think that nothing about the size of the Clinton/Bush effect could be determined without the unconfirmed nominees." *Id.* at 265. "Adding in the unconfirmed nominees, a factor of which Saks and Vidmar make so much in their response, changes the overall results not a whit. Although the dataset does not include prior judicial experience, with an identical overall effect size, there is no reason to think that the strong political effects that were concentrated among those with prior judicial experience [sic – this statement gives the results in reverse] in my original study would go away. Even if they did, the effect size is now significant without splitting the file." *Id.* A page later, he again mischaracterizes the critique as, "[a]ll of the 'sturm und drang' about failing to include unconfirmed nominees." *Id.* at 266.

[18] See the immediately preceding paragraph.

[19] "Further, it is common for scholars to base their statistical analyses on only confirmed federal judges, as I did." Lindgren Rejoinder, *supra* note 2, at 260.

[20] Contrast Professor Lindgen's overblown conclusions with those of Susan Brody Haire, who also conducted research on ABA ratings. Susan Haire, *Rating the Ratings of the American Bar Association Standing Committee on Federal Judiciary*, 22 JUSTICE SYSTEM J. 1 (2001). Professor Haire used some variables that were similar to those used by Lindgren and examined ABA ratings of Court of Appeals nominees from the Carter, Reagan, Bush and Clinton administrations. She

question is not: "Why should my study be criticized when the studies of others were not?" The proper question is whether the criticism is sound and the conclusions of the study are not. The empirical research enterprise is about building a body of sound knowledge; it is not about setting standards of acceptable mediocrity. If the other studies drew flawed conclusions because their populations or samples were not up to the task at hand, then the criticism applies to them as well.[21]

Professor Lindgren also argues that the proper target population is whatever the researcher says it is. He argues that he, not we or anyone else, get to say what his study's target population should be.[22] He is mistaken. A researcher is entitled to choose the research question he wishes to study. But once a research question has been framed, the appropriate target population is not whatever the researcher declares it to be but, rather, what will serve best to provide a valid answer to the research question. The choice of a proper target population is an issue

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concluded that appointees with greater legal and judicial experience tended to receive higher ratings and that white males were more likely to receive higher ratings than females and minority group members. *Id.* at 14-15. While noting the potential policy implications of her findings, she cautioned:

> The conclusions offered here are subject to future research. Experience levels were found to be determinants in the ratings given, but the potential for biased evaluations remain. Without information from the standing committee on the rationale underlying a judge's rating, one can only speculate, for example, on the causes underlying gender and race differentials and whether those differences stem from systematic variation in the weight accorded to particular professional experiences. Additional studies should continue to build on these modest findings and explore further the bases of the ratings decisions.

Id. at 15. We believe that some of Professor Haire's variables (e.g. gross rankings of law schools) are subject to one of the criticisms that we offered concerning Professor Lindgren's data. Nevertheless, as the above quotation suggests, Professor Haire clearly noted the serious limitations of her data.

[21] This principle is broadly recognized in the sciences and social sciences. *See, e.g.*, Seymour Sudman, *Applied Sampling, in* HANDBOOK OF SURVEY RESEARCH 149 (Peter Rossi et al. eds., 1983); Federal Judicial Center, REFERENCE MANUAL ON SCIENTIFIC EVIDENCE, at 239-247. It also has been recognized by the courts. *See, e.g.*, Snodgrass v. Ford Motor Co., 2002 WL 485688 (D.N.J. 2002) (finding that the database that was the foundation for the expert's analysis was of uncertain validity, and therefore any conclusions based on it were inadmissible).

[22] Lindgren Rejoinder, *supra* note 2, at 275 ("It is rare that one researcher would try to tell another what his target population is, but Saks and Vidmar seem to want to do this.").

that can be analyzed and criticized.[23] Better and poorer choices can be made in light of the research question that is being asked.[24] In this case, the original article never comes right out and states a precise research question. But it plainly is about the allegation of bias in the ABA's evaluations of prospective nominees.[25] Given that, we reasoned that the most appropriate target population would be the prospective nominees who are subject to the ABA's evaluations. And we pointed out that Professor Lindgren's sample[26] missed that by a wide margin with potentially serious consequences for his conclusions. Professor Lindgren gives no rationale for the suitability of the population from which he did sample. We suspect it was chosen for the same reason the control variables were chosen: maximum convenience to the researcher.[27]

As to the omission of data on district court judges, if the research question was the rather general one of whether or not ABA evaluations are politically biased,[28] then it is reasonable to suppose that the ABA's evaluations of prospective district court nominees—in addition to prospective circuit court nominees—would be another source of data on which to test that hypothesis. Professor Lindgren now posits a more refined hypothesis that seems to have grown out of his study and our critique of it rather than being the hypothesis underlying it. He says that his "thesis is that the Clinton/Bush effect is not across the board, [so that] examining the district court nominees for possible ABA bias

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[23] *See* Shari S. Diamond, *Survey Research: Was an Appropriate Population or Universe Identified*, § 7-3.1, *in* MODERN SCIENTIFIC EVIDENCE (David Faigman, David Kaye, Michael Saks, and Joseph Sanders, eds., 2002).

[24] For example: Suppose you sue someone for infringing the trademark you own for a product. And suppose you conduct a survey of consumer confusion among users of the product. But suppose that for this particular product, the usual users are not the same people who make the purchases. The other party is entitled to criticize your choice of a population, and the court will decide how much your poor choice of a target population distorts the findings, and as a result might discount your findings or disregard your data entirely. *See id.* and cases cited therein.

[25] *See* Lindgren Study, *supra* note 3, at 3 ("the hypothesis that there is bias"). *See also supra* note 15.

[26] Professor Lindgren prefers to call this the "sampled population." Lindgren Response, *supra* note 2, at 265. In doing so, he moves the focus of criticism from poor execution (the sampling frame missed the target population) to poor conceptualization (he chose the wrong population).

[27] The convenience of the Federalist Society researchers who created the dataset as well as the convenience of Professor Lindgren who was the recipient of the data. *See* Lindgren Study, *supra* note 3, at 1 n.1, and at 4-5.

[28] *See supra* note 15 and accompanying text.

would be interesting or enlightening, but it is not part of the thesis."[29] A reader of the original article would not have known that the bias hypothesis was from its outset so limited and precise. In trying to explain the basis for this new hypothesis, he says, "[T]he ABA Standing Committee uses different stated standards for district court judges, [the district judges'] appointments are much less influential to a president's agenda, the stakes are lower, and senatorial courtesy is much more often observed."[30] Nowhere does Professor Lindgren suggest how any of those factors are relevant (or, as he implies, irrelevant) to the bias hypothesis as that hypothesis might be tested by studying ABA evaluations of prospective district court nominees (or sitting district judges).

If Professor Lindgren had a narrower research question or hypothesis, that is fine. But if that were so central to his thinking, as he now claims, it would have been useful and informative for him to have explained with some care why one should expect bias aimed at a subgroup of prospective appellate court nominees, yet not for other prospective appellate nominees and not for any prospective district court nominees. Perhaps he has avoided this issue because of difficulty in proposing a credible psychological explanation. Indeed, what could the psychology behind such a prediction be?[31] He offered no explanation in the original article and offers no explanation now. Moreover, if his thesis really is so narrow, and his findings are so narrow, then why aren't his conclusions similarly narrow?[32]

C. There is Equally Strong Evidence of Non-bias, Which Professor Lindgren Only Partially Shares with Readers.

An analysis of the total sample employed in Professor Lindgren's study—both nominees with judicial experience and those without such

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[29] Lindgren Rejoinder, *supra* note 2, at 275.

[30] *Id.*

[31] Imagine if one tried to instruct the ABA committee on how to target the application of their biases: Here's what we're going to do. We're going to allow our political biases to run rampant in reaching our judgments of prospective appellate court nominees without prior experience as state judges or federal district judges, but we won't let it affect our judgments of those prospective appellate court nominees *with* prior judicial experience. And we won't let it affect our judgments of prospective district court nominees, because they aren't important to the president's agenda and senatorial courtesy is more often observed. Everybody got all that?

[32] *See supra* note 6 and accompanying text.

experience—showed no significant difference between Clinton and Bush nominees at the conventional level of significance.[33] Professor Lindgren did not report that non-difference at all. An analysis of the 55% of the sample who had any kind of prior judicial experience shows an overwhelming lack of differential effect as a function of which president was doing the nominating. Professor Lindgren sets these data aside early in his article, hardly referring to them again, and never allows them to temper his sweeping conclusions. An analysis of those nominees *without* prior judicial experience shows higher ratings for the Clinton nominees. Amplification of this finding occupies the bulk of Professor Lindgren's article.

Part of our criticism is that the pattern of data raises doubts that what is going on is in fact political bias. This is at once a very simple and a very puzzling picture: apparent bias in ratings of one group; no apparent bias in ratings of the other group. What might explain that? Professor Lindgren made no effort to offer an explanation, plausible or otherwise, in his original paper or his rejoinder. He asserts the claimed phenomenon repeatedly; but he does not attempt to make sense of it.

Imagine a study of police officers' hypothesized bias toward black versus white citizens. Imagine that a study is conducted which finds evidence consistent with the hypothesis of bias on odd numbered days, but the data from even numbered days show no differential treatment. Apparently, Professor Lindgren would say that such a pattern of data proves the police force is biased. It need not be present on all days; half are enough; no deeper inquiry is called for. (Query: For Professor Lindgren, would one day a week suffice, and still not rouse his curiosity about what could explain such a pattern? How about one day a year?) Professor Lindgren is content to say that it is because life is complex and full of interactions among variables.[34] We say that such a pattern is so odd and counterintuitive that acceptance of it should await replication using better samples and better measures, or at least until a plausible explanation for it can be proposed. There has to be an explanation for such a pattern, and the pursuit of an explanation might either illuminate the nature of the bias or reveal that what looks like bias is really a reasonable response to something else, something related to the merits of the prospective nominees.

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[33] *See* Saks & Vidmar Critique, *supra* note 4, at 227 n. 22.

[34] *See* Lindgren Rejoinder, *supra* note 2 at 269.

That the interaction effect involves groups of nominees rather than days of the week only makes it seem that we can understand such an odd pattern. But, as we noted earlier, no explanation for it has been put forward by Professor Lindgren. What might explain the pattern of his data?

One of the least plausible explanations is that a generalized political bias can be so neatly patterned, so precisely deployed under certain conditions and not others.

Since life is full of confounds (as well as of the interactions that Professor Lindgren highlights), it is possible that being a judge versus not being a judge is confounded with something else that has an impact on the evaluations of prospective nominees. Judges have already jumped through numerous hoops to get where they are; that process may tend to filter some things out or some common elements in. Those who have not already jumped through those hoops are likely to be a group more heterogeneous on some variables of importance. What might those be? Perhaps these have to do with the core qualifications of promise as a federal appellate judge. Professor Lindgren made an attempt to control for the qualifications, but (as we remind the reader below) we explained in considerable detail why that effort was crude and ineffective. So it remains possible that what caused some nominees to receive higher ratings than others was the constellation of attributes that qualify one for the job under consideration.

Perhaps the observed pattern is the result of selection decisions made by the White House as they vetted the pool of prospective nominees down to the pool of nominees, such that whatever explains those decisions explains the pattern eventually observed—a pattern that did not exist at the stage of ABA evaluations of prospective nominees. This is one result of missing the correct target population.

At bottom, our criticism on this issue was that Professor Lindgren highlights the evidence supporting the bias hypothesis while sliding the other evidence into the background, where it would appear not to exist or to have no relevance. He argues that the effect is in the interaction (between the variables of prior judicial experience and the nominating president). Fair enough. But then he has no warrant for failing to make the data at one level of the interaction as salient as the data at the

other level.[35] And no warrant for converting an interaction effect into something that sounds like a main effect.[36] He shouts loudly about the portion of the pattern that he likes, and whispers about the portion that he does not like.[37]

D. The Control Variables Used to Account for Differences in Nominees' Qualifications Are So Deficient as to Be Useless.

This is the most fatal weakness of Professor Lindgren's data. A finding that some Clinton nominees earned higher grades from the ABA than Bush nominees has an obvious possible explanation, namely, that the Clinton nominees, on average, had superior qualifications. Professor Lindgren purports to control for this possibility, but employs ineffective control variables. The bulk of his analysis is devoted to myriad permutations of models employing those ineffective control variables. Our essential criticism is that his effort at control was so crude as to be meaningless, and therefore his conclusions are meaningless.

Professor Lindgren measured only a small portion of what the ABA says it is measuring.[38] And the small portion that he measured used

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[35] It also raises the question of why one would split a sample instead of including the interacting variable in the models. There is no analytic reason for doing so. It is better to keep important interactions in one's analysis so that the form of the interaction, and therefore the nature of the phenomenon, can be better understood. And throwing away 55% of the sample makes readers forget that it — and the puzzle it raises — exists.

[36] *E.g.*, Lindgren Study, *supra* note 2, at 26 ("the patterns revealed in the data are consistent with a conclusion of strong political bias favoring Clinton nominees," "an evaluative process that seems to be so strongly biased."). But the most important way in which the interaction is hidden is that the data set is split, so that the 55% of nominees with prior judicial experience simply disappear from the analyses, so that so much of the analysis and discussion leads a reader to the impression that the effect, if there is an effect, is a general one.

[37] Professor Lindgren's assertion that we believe that an effect has to be everywhere or it is nowhere is an inaccurate characterization of what we do say. *See* Lindgren Rejoinder, *supra* note 2, at 269 (asserting that Saks & Vidmar have argued, in effect, that "either all variables always have the same effects in all situations as all other variables, or at least bias always has the same effect in all situations as another variable.") (On the other hand, it appears that Professor Lindgren thinks that if an effect is found anywhere he can talk as though it is present everywhere.) What we have been arguing is that the pattern makes the effect doubtful and the data support only a tentative indication that requires more research to figure out what is going on. And that an author of such work has a responsibility to be more forthcoming with readers, enabling them to see the uncertainties as well as the possible implications of the evidence.

[38] This is the problem of construct under-representation. *See* Saks & Vidmar Critique, *supra* note 4, at 231-37.

criteria that were unlikely to provide a meaningful index of "qualifications." In our critique, we discussed each control variable and its shortcomings for use as a measure of quality *within this rarified population* in quite some detail.[39] We explained that to expect these crude measures to distinguish better from less well qualified prospective federal judges would be like expecting to sort a group of Nobel science laureates using such variables as whether they have a Ph.D., spent a lot of time in laboratories, or subscribe to *Science*.[40] The point was that for a group already screened to find very highly qualified people, these measures are too basic, too primitive, too crude. And in fact these measures have no relationship to the ABA ratings.[41] In other words, they are incapable of serving the purpose for which they are employed in the

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[39] Professor Lindgren's study compared nominees according to whether they attended a top ten law school, served on law review, had a judicial clerkship, worked as a private lawyer, and worked as a government lawyer. Lindgren Study, *supra* note 3, at 6-15.

[40] To make the variables more directly analogous: whether they attended a top ten doctoral program (by the lights of *U.S. News*), served as an editor of a science journal, apprenticed for a top scientist, and worked in a private or university laboratory. The point is that it doesn't matter how they scored on these variables; they already have demonstrated their excellence in more concrete ways, so that these measures will be unable to differentiate among them.

[41] We now have performed a discriminant function analysis to see whether Professor Lindgren's five "control" variables can distinguish between those who win unanimously well qualified ratings from the ABA and those who are rated anything less than that. There is no relationship. (For the sample of nominees without prior judicial experience: Wilks' Lambda = .935, $\bullet^2 = 3.001$, $df = 5$, $p = .699$. For the sample of nominees with prior judicial experience: Wilks' Lambda = .892, $\bullet^2 = 6.22$, $df = 5$, $p = .285$.)

Professor Lindgren might argue that this shows how truly biased the ABA is: they totally ignore these indicia of qualifications in their evaluations of prospective nominees. But such an explanation requires one to believe not only that the allegedly biased committee members allow political considerations to color their judgments, but also that they pay no attention whatsoever to any real criteria. These data are far more consistent with the more plausible suggestion that those who reach the stage of serious consideration for a seat on the federal bench are already sufficiently highly qualified that these measures have no bite.

On the other hand, in partial support of what these measures were intended to evaluate, we did find the following oddity from simple frequency counts. *See* Saks & Vidmar Critique, *supra* note 4, at 249 n.78. Concerning the group of 49 successful nominees without prior judicial experience: of the 19 with the weakest qualifications using Professor Lindgren's five measures, 56% were Bush nominees while only 29% were Clinton nominees. At the other end of the spectrum, of the 18 with the strongest qualifications, only 28% were Bush nominees while 42% were Clinton nominees. But these were not statistically significant differences. Still, in an exploratory study such as Professor Lindgren's they would lead a curious researcher to wonder if more relevant measures of qualifications would cause the apparent bias effect to shrink or even to disappear. Professor Lindgren offered no answer to this tantalizing clue.

study.[42] Furthermore, we asserted the obvious: None of these measures was selected because it was determined to be an appropriate and valid measure of "judicial qualification" in this context. Each was chosen because it was cheap and easy to obtain. This is what is known in the research business as "quick and dirty."[43]

Professor Lindgren's rejoinder offers no substantive defense to this fatal criticism. In his original paper he argued that the things he failed to measure were incapable of being measured empirically.[44] That is plainly incorrect,[45] and he no longer presses the argument. In his rejoinder he now says that he acknowledges that some variables went unmeasured, but that is always a danger in correlational research, and that since everyone runs into that problem, it is no failing that he did also. "What I object to is the implication in the Saks/Vidmar response that there is something special about the omitted variable bias problem in my study."[46]

What Professor Lindgren fails to see or refuses to recognize is that in his study the problem is not that some unanticipated and perhaps minor explanatory variable went unmeasured and omitted from the model. The problem for his study is that he did not even begin to adequately measure the most relevant and obvious control variable that would have tested the most obvious alternative explanation for the apparent differential treatment of nominees. As a consequence of this, the null hypothesis of no actual difference in ABA ratings of equally qualified nominees remains viable and plausible because it remains untested. Professor Lindgren himself noted that, "[w]ithout controlling for background credentials, one cannot make even a good circumstantial case for the existence of political bias in rating the

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[42] To put the point more simply: Our argument is that *for this highly pre-screened population*, Professor Lindgren's measures were no better than using such variables as whether a nominee prefers dogs or cats and whether the nominee usually wears blue or white shirts. If these variables do not affect the outcome of interest, they won't be effective controls.

[43] We feel comfortable making this assertion because we have seen this practice an uncountable number of times in other shoddy studies. Normally one does not so much defend a quick-and-dirty study as to hang one's head sheepishly and mumble an apology. Or else to present it with the humility and caveats that it requires. Many researchers have done a few quick-and-dirty studies in their careers. Few try to make so much of them or defend them so forcefully.

[44] *See* Lindgren Study, *supra* note 3, at 4 ("Some of the ratings criteria, however, cannot be measured empirically, such as integrity and judicial temperament.")

[45] *See* Saks & Vidmar Critique, *supra* note 4, at 236 n.40.

[46] Lindgren Rejoinder, *supra* note 2, at 259.

qualifications of judges."[47] Using ineffective control variables is the same as doing the analysis "without controlling for background credentials."

Since all of Professor Lindgren's analyses are conducted within a framework that suffers from this fundamental problem, no amount of number crunching with those quick and dirty and useless measures can tell us anything about whether the difference in ratings is due to bias on the part of the ABA evaluators or whether it is due to differential qualifications among the candidates.

There is independent evidence at least suggesting that the Bush Administration did not select its judicial nominees with as much care as other administrations, thereby making the untested alternative hypothesis quite plausible.[48] Long before the current controversy about ABA ratings erupted, Professor Sheldon Goldman wrote about the first President Bush's judicial legacy.[49] In an article published shortly after President Bush left office, Professor Goldman noted that

> The argument can be made . . . that the Bush administration failed to invest sufficient resources in judicial selection. The Reagan administration operated a separate Office of Legal Policy led by an assistant attorney general to handle judicial selection. In contrast the Bush administration first employed an assistant to the attorney general and then a deputy assistant attorney general who worked full time in the Civil Rights Division to handle judicial selection.[50]

The White House Counsel's office also screened candidates, but ordinarily did not personally interview them. Then, when a prospective nominee was selected, the ABA Standing Committee on Federal Judiciary was given the name and asked for its rating.[51]

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[47] Lindgren Study, *supra* note 3, at 3.

[48] We cited and summarized this evidence in our Critique, but it deserves to be presented again and more prominently.

[49] Sheldon Goldman, *Bush's Judicial Legacy: The Final Imprint*, 76 JUDICATURE 282 (1993).

[50] *Id.* at 285.

[51] *Id.*

While some of Goldman's discussion takes place in the context of delay of judicial appointments, it suggests that, in contrast to the Reagan administration, less time and fewer resources were devoted to judicial selection during the Bush administration. If so, perhaps any differences between Bush and Clinton can also be attributed to the first President Bush's poorer selection processes.[52] This is not, of course, to say that this alternative hypothesis is true. But the hypothesis that the two pools of prospective nominees differ in their real qualifications enough to explain any differences in ABA ratings is so obvious *a priori*, as well as being supported by circumstantial evidence, that it needs to be tested with some seriousness. The Lindgren Study did not do so, nor is it properly entertained in the rejoinder.

E. The Proposed Solutions Do Not Follow from the Asserted Findings

We explained that, although Professor Lindgren views his bias study from much the same perspective that one views a Title VII employment discrimination study, his implicit analogy misconceives the federal judge "hiring process." His notion is that since he (thinks he) has found evidence of bias in ABA evaluations (among one subgroup of prospective judicial nominees), the ABA should either cease the alleged bias or be excluded from the process.[53] We explain that even if it were true that the evaluations of the prospective nominees of one party were unjustifiably elevated over those of the other party, because we have only one president at a time, the bias makes no difference to the "hiring process."[54] Under such circumstances, a rational researcher (and a rational president) would not choose to ignore the ABA ratings, but would simply want to continue skimming the cream off the top, whether that cream comes labeled "unanimously well qualified" or only "majority well qualified" or whatever. The solution is to keep selecting the highest rated people. So long as the *rank order* of prospective nominees

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[52] *See also*, W.G. Ross, *Participation by the Public in the Federal Judicial Selection Process*, 43 VAND. L. REV. 1 (1990) (making the point that judicial selection procedures have varied by presidents, depending on the administration and its relationship with Congress), whose conclusion seems consistent with Goldman's observations.

[53] This is not to say that there are not reasonable arguments why the ABA ought not to be given any special role in the judicial selection process. But none of them have anything to do with Professor Lindgren's Study, whether or not its findings are sound.

[54] *See* the more detailed discussion in Saks & Vidmar Critique, *supra* note 4, Section VII. Remedies.

submitted by any given president did not change, it would not matter whether their "grades" were shifted up or down due to some bias unrelated to ability.[55] If, however, the bias led to giving higher ratings to less competent candidates and lower ratings to more competent candidates *within* a single president's group of prospective nominees, then the ratings would be sending misleading signals (saying falsely that weaker candidates *within* the group being compared and decided upon are stronger, and stronger candidates within the group are weaker). But, as *between* administrations, the sorts of differences Professor

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[55] In his rejoinder, Professor Lindgren supplies new data concerning the relationship between the ABA ratings and the likelihood that a nominee will be confirmed. *See* Lindgren Rejoinder, *supra* note 2, Table 1, at 264. For the reader's convenience, we reproduce the table which reflects the ABA ratings given to both confirmed and unconfirmed federal appellate nominees from 1977–2000:

Table 1
Probability of Success in Confirmation to the U.S. Court of Appeals
by ABA Rating, 1977-2000
Primary Data Source: Michigan State University; n=312

ABA Rating (majority/minority)	% Nominees Confirmed
Well Qualified (or higher) (n=165)	85%
Well Qualified/Qualified (n=35)	71%
Qualified/Well Qualified (n=15)	80%
Qualified (n=65)	71%
Qualified/Not Qualified (n=32)	75%

Professor Lindgren asserts that "the relationship is not primarily a linear one; it is more of a cliff—if you fall below unanimously 'Well Qualified,' it matters little which other rating you have." Lindgren Rejoinder, *supra* note 2, at 264. A cliff? Really? Take a good look at the data. Doesn't this look more like rolling hills on a high plateau? Isn't this an odd pattern? Or, we might say: is this a pattern? Not only is it "not primarily linear," it is not linear at all. (An unweighted means analysis of variance of the linear component of these data shows no significance: $F=1.312$, $df=1.307$, $p=.253$.) It is ups and downs with no overall trend. In fact, what you would expect to be at the bottom of the cliff ("Qualified / Not Qualified") does not differ in elevation from the top of the cliff! (Comparing these two groups, Fisher's Exact Test gives $p=.196$ (two-tailed) and $p=.135$ (one-tailed).) That is to say, those with the *lowest* rating are as likely to be confirmed as those with the *highest* rating. The non-pattern here is that there is no relationship between how the ABA rates a nominee and whether the Senate confirms the person. At the same time, approximately four-fifths of all the nominees are confirmed.

So, here is another empirical puzzle about which Professor Lindgren shows no curiosity or even awareness. Yet it cries out for understanding and no doubt is an important clue to how the federal judicial selection process works.

Lindgren thinks he has discovered are simply irrelevant to the selection process.[56]

Professor Lindgren still offers no answer to this criticism.

II. VARIOUS AND SUNDRY ISSUES AND ACCUSATIONS

In his rejoinder, Professor Lindgren raises a large number of subsidiary, tangential, or irrelevant points. Space, time, and mercy restrain us to answer only a few of them.

A. The Emory Article on Juries

In his reply Professor Lindgren devotes two full pages to the methodological analysis of an article in the *Emory Law Review* by one of the present authors, Vidmar, dealing with jury verdicts in civil cases.[57] Professor Lindgren's purported purpose for his digression onto an entirely different topic is to "get some perspective"[58] on unmeasured variables.[59] The basic reasoning of Professor Lindgren seems to be that

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[56] For the reader who is finding this abstract description puzzling, consider this analogy: Imagine that the Federal Judicial Center told two different law schools that it wished to reward each school's five best students in their respective courses on judicial administration with a free trip to Washington to serve as "judges" of a courthouse art and architecture fair. And suppose that one of the professors is an easier grader than the other, so that his students each received ten points more in the course than they would have received if they had been attending the same course in the other school. Indeed, the grades given by that professor in that course averaged ten points higher than the grades given by the other professor in the other school. That bias would have no effect whatsoever on the selection of which students were to become the "judges." That kind of bias produces no unfairness or distortion in the selection of the five winners from each school. This is the kind of situation that Professor Lindgren complains of, but it should now be clear that the complaint (even assuming the bias exists and is strong) is simply wrongheaded, because it has no effect on the resulting selections. On the other hand, if one of the two professors in the hypothetical illustration gave higher grades to his weaker students and lower grades to his stronger students, then one could complain that the wrong people were being selected to become "judges." That is the kind of situation which should arouse concern. But it is not at all what Professor Lindgren complains of or says that he has found any evidence of.

[57] Neil Vidmar, *Scientific and Technological Evidence: Are Juries Competent to Decide Liability in Tort Cases Involving Scientific/Medical Issues? Some Data From Medical Malpractice*, 43 EMORY L.J. 885 (1994) (hereinafter Vidmar). Discussed in Lindgren Rejoinder, *supra* note 2, at 258-59.

[58] Lindgren Rejoinder, *supra* note 2, at 258.

[59] Although Professor Lindgren states that his "intent is not to criticize Vidmar's *Emory* study," *id.* at 258, he does so and does so repeatedly. At 258: "Unlike those in my study, the conclusions of Vidmar's are likely to be affected by selection bias." At 260: "Again, if Saks and Vidmar had said that one should be cautious if a study does not have some of the cases that one might be interested in, but that the problem here was less serious than in Vidmar's study." And at 286: "But instead of

Vidmar made major errors of reasoning and thus he, Professor Lindgren, should be given leeway for the lapses in his article on ABA ratings. This digression to Vidmar's article might arguably be acceptable—or not, since it distracts the reader from the issue at hand, namely, problems with Professor Lindgren's data. But an unmistakable problem with Professor Lindgren's digression is that it seriously misdescribes the purpose of Vidmar's article.

Just as Saks and Vidmar pointed out that Professor Lindgren went beyond his data and made unwarranted claims, the clearly indicated purpose of the Emory article was to caution against unwarranted claims and failure to consider plausible alternative hypotheses. In the article Vidmar drew attention to the fact that critics of the jury system had made claims about jury bias, incompetence, and irresponsibility based on anecdote, absence of data, misinterpretations of statistical data, failure to consider plausible alternative hypotheses, and ideology. The Emory article consisted of a review of empirical findings, some of which were collected by other authors and published in peer-reviewed journals, data which offered more systematic and thorough evidence on those issues of jury decision-making.

To make the importance of Professor Lindgren's mischaracterization of the Emory article clear, note that when a hypothesis—for example, "X is greater than Y"—is scientifically tested, the scientist tests it against a "null hypothesis" that there is no difference between X and Y.[60] If no statistically significant differences are found between the distributions of X and Y, then it is not correct to conclude that X and Y are different. However, if the data actually show the opposite, that Y is greater than X, then the hypothesis is in serious trouble because it is contradicted. The Emory article drew attention to the fact that jury critics made claims that juries behaved in a certain way, essentially a hypothesis. The data reviewed in the article showed that in actuality there were

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noting that my omitted variable problem was less serious than in some of their own work (such as Vidmar's 1994 Emory study)...."

Also note that, although Professor Lindgren does not even cite a single study by Saks, he nevertheless asserts that "my omitted variable problem was less serious than in some of *their* own work...." (emphasis added). If Professor Lindgren were writing this reply, he would at this point devote several paragraphs if not pages to arguing that this unfounded charge is a well-known stratagem intended to discredit Saks without offering an iota of evidence. We will attribute it, instead (and far more briefly) to minor sloppiness.

[60] Federal Judicial Center, REFERENCE MANUAL ON SCIENTIFIC EVIDENCE, 123, 167.

empirical research findings pointing in the opposite direction to the critics' hypothesis. No claim was made that these data were definitive, but they did go in the opposite direction of the asserted hypothesis. Thus, in contrast to Professor Lindgren's characterization of missing cases in Vidmar's review, the data *were* the missing cases—or at least some of them—that showed that the hypothesis was contradicted.

Equally important, the data summarized in Vidmar's article were discussed with strong and clear qualification. For example: "While we must consider the possibility that these jury critics could ultimately prove to be right, careful analyses indicate that to date they have produced no evidence to support their claims that withstand scientific scrutiny."[61] Relatedly, Professor Lindgren's discussion of the Emory article, fails to mention that the article specifically made reference to extensive articles, one by Saks[62] and one by Vidmar,[63] that are devoted to exploring and explicating methodological problems and that point out potential errors of missing cases and other selection problems. It is an interesting oversight.

B. The Claim that the Only Proper Way to React to a Study is to Offer New Data or New Analyses of One's Own

Professor Lindgren complains: "In litigation, people often take the Saks/Vidmar approach—trying to tear down the other side instead of adding to the store of knowledge. In social science, on the other hand, the standard practice is to try to offer proof for speculations."[64] "The better approach is actually to obtain data."[65]

Professor Lindgren is correct about the long run: except for devout theoreticians, the great majority of social scientists believe that the road to knowledge is paved with more and better data. But they also believe

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[61] Vidmar, *supra* note 57, at 888. And in the closing the caution was repeated: "To be sure, until recently good data have been lacking and still more are needed to get a veridical picture of jury performance in tort cases. . . . Nevertheless, this article should raise some doubts of extravagant claims of jury bias and incompetence, about the need for radical reforms, and about the calls for more judicial intervention." *Id.* at 911.

[62] Michael J. Saks, *Do We Really Know Anything About the Behavior of the Tort Litigation System—and Why Not*, 140 U. PA. L. REV. 1147 (1992).

[63] Neil Vidmar, *Making Inferences about Jury Behavior from Jury Verdict Statistics: Cautions about the Lorelei's Lied*, 18 L. & HUMAN BEHAVIOR 599 (1994).

[64] Lindgren Rejoinder, *supra* note 2, at 260.

[65] *Id.* at 261.

it is paved with careful thought. And to achieve that, the totality of the enterprise involves more than the production of new empirical data. In addition to empirical studies, there also are evaluative literature reviews, integrative literature reviews, critiques,[66] theoretical integrations, commentaries, symposia, book reviews,[67] and everything else that is part of the (post publication) peer review process. Social scientists also critique manuscripts reporting research when they serve as editorial consultants or editors of journals which publish empirical research, or as members of dissertation committees, or funding agencies reviewing research proposals, or hiring and promotion committees, or as scholars asked by a colleague to critique the colleague's draft manuscript. In this instance, we critiqued a study. However much Professor Lindgren may wish that were not a proper part of the process of growing sound knowledge, it unequivocally is.

C. Conventions for Referring to the Intellectual Parents of Studies and Other Articles

Professor Lindgren complains that

> Another twisted argument is Saks and Vidmar's repeatedly referring to my study, which was funded by the Searle Fund at Northwestern University, as the "Lindgren/Federalist Society Study." In [an earlier] draft of their article . . . they referred to my study in this way throughout the text and even in the title of their article. References to the "Lindgren/Federalist Society Study" were reduced to only 14 [in their final version]. I doubt that those who actually funded my study, the Searles or their private foundations, appreciate scholars attributing the study to a different sponsor. . . . Instead of referring to it as the Lindgren/Searle study or Lind-

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[66] *See,* e.g., another recent critique of an empirical study, invited as part of a symposium. Michael J. Saks, *Trial Outcomes and Demographics: Easy Assumptions versus Hard Evidence,* 80 U. TEX. L. REV. 1877 (2002).

[67] *See, e.g.,* Michael J. Saks, *Medical Malpractice: Facing Real Problems and Finding Real Solutions,* 35 WILLIAM & MARY L. REV. 693 (1994) (Reviewing WEILER ET AL., A MEASURE OF MALPRACTICE: MEDICAL INJURY, MALPRACTICE LITIGATION, AND PATIENT COMPENSATION (Harvard University Press, 1993)).

> gren/Northwestern study, Saks and Vidmar repeatedly call it the Lindgren/Federalist Society study. [68]

The convention is that studies are typically referred to by their authors' names, and authors are those who made substantial intellectual contributions to the project, and rarely by the sponsors that funded the project. In this case, we attempted to give proper credit to those who did substantive intellectual work on the research, namely, Professor Lindgren and persons at the Federalist Society. The sponsors do not do the intellectual work of a study, so they rarely become the reference tag. The Federalist Society collected and coded the data to be used in the study, not Professor Lindgren, though he double-checked a portion of their data.[69] They were not acting as his research assistants, working under his supervision. They were the intellectual parents of the dataset.[70] If Professor Lindgren thinks that such coding and who does it have no intellectual component and no consequences for the study, he is doubly mistaken.[71] One is responsible for one's intellectual creations. An editor suggested to us that, since the Federalist Society's intellectual contribution was confined to data collection and coding, that we would be more accurate if we limited the references to the "Lindgren/Federalist Society Study" to those occasions where we were referring to work of the Federalist Society and for which they might properly receive credit or criticism. We readily adopted this sensible suggestion. In the end, our references to the Federalist Society as an intellectual parent of the study tracks as closely as possible their role in the project as that was described by Professor Lindgren.

On the other hand, we do not refer to our paper as the Saks & Vidmar & ABA critique, because no one at the ABA played the slightest role in the development of any of the ideas or opinions presented in the article.[72] The ABA, through an academic intermediary, approached us

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[68] Lindgren Rejoinder, *supra* note 2, at 278.

[69] *See* Lindgren Study, *supra* note 3, at 4-5.

[70] If one obtains a pre-created, public dataset, such as from the Census Bureau or the Administrative Office of the United States Courts, the convention is simply to acknowledge the source, because it is used by many researchers.

[71] *See* ROBERT ROSENTHAL, EXPERIMENTER EFFECTS IN BEHAVIORAL RESEARCH (rev. ed. 1976).

[72] *See* Saks & Vidmar Critique, *supra* note 4, Author Note. As we noted in that earlier paper, we were, however, compensated for the time we spent reviewing the Lindgren Study and writing that critique. *Id.* We have received no compensation for writing the present reply.

to review Professor Lindgren's study. We agreed to do so. Then all of those people abandoned us to our task. The ABA played no part whatever in the thinking or writing or editing of our paper. When we say abandoned, we mean abandoned. At one point, when the two authors could not resolve a disagreement about some point, we sought the mediation of the academic intermediary. He refused. He gave us no more assistance in resolving our substantive disagreement than a judge who tells a jury to "get back in there and keep arguing with each other and try to sort it out." In addition, when Professor Lindgren writes: "I can't think of a worse strategy for the ABA, an organization trying to create the impression that it is not politically biased, than using language that would create the false impression that the Federalist Society directed or commissioned my work on this project,"[73] he implies that the ABA played a part in writing our paper: it was "the ABA . . . using language." Whatever may be the reason Professor Lindgren wrote such a statement, despite no lack of notice that it was untrue, he is completely incorrect.[74]

D. The Charge of Fabrication

We are accused of a fabrication, centering around the phrase "hallway conversation."[75]

> Sometimes this same practice of twisting the facts caused Saks and Vidmar to cross the line into statements for which there is no basis at all. For example, Saks and Vidmar stated in the draft I was replying to: "The only evidence or reasoning in support of Professor Lindgren's interpretation is a hallway conversation with a colleague." After I submitted an earlier draft of this rejoinder, the editors requested that they delete this factually inaccurate statement, which they did . . . .

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[73] Lindgren Rejoinder, *supra* note 2, at 279.

[74] If the ABA were writing this, they would have been more interested in pointing out that by writing, "the ABA, an organization trying to create the impression that it is not politically biased," Professor Lindgren has betrayed his own prejudices, namely, his presumption of ABA bias, which calls into doubt his own insistence that he is just a social science researcher disinterestedly looking for an empirical answer to an empirical question.

[75] This is undoubtedly the silliest thing we have ever had to respond to in our professional lives. Nevertheless, we will discuss it with more sobriety than it deserves.

> They just fabricated this claim . . . which they have now effectively admitted by dropping it.[76]

This is not a fabrication. This is what is known as a figure of speech. A "hallway conversation" is like a "curbstone opinion" or a conversation "over the backyard fence" or "around the water cooler." None of these expressions means literally that the exchange took place at a curbstone or over a back fence or around a water cooler—or in a hallway. Each of them puts a certain gloss on the exchange to which the writer is referring. A "hallway conversation" for academics is much like a "curbstone opinion" for lawyers: something quite preliminary, not yet properly analyzed, not something you would want to rely on for a serious purpose.

By saying that the "only evidence or reasoning in support of" something was "a hallway conversation," we were saying in shorthand what we also said more specifically: that there was little if any analysis, evidence, reasoning or much other basis for the point. In Professor Lindgren's original study this point appeared near the end of the article, a small bit of information that he considered confirmatory of what he claimed his data showed.[77] In his rejoinder it takes on a far more important role. Now we are told that the findings were not merely the result of an exploratory look to see whether the data confirmed or refuted accusations of bias, but that they confirmed a specific hypothesis that he had formed going into the study that predicted the very interaction that he found.[78] And still he offers no

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[76] Lindgren Rejoinder, *supra* note 2, at 279.

[77] Lindgren Study, *supra* note 3, at 28-29. He now tells us that this same belief ("that nominees with prior judicial experience had a better chance of getting through the Senate and the ABA") was shared by "several people who were involved in judicial selection during the Reagan, Bush, and Clinton administrations, as well as Nina Tottenberg (*sic*)" and that it was "widely discussed at the time." Lindgren Rejoinder, *supra* note 2, at 279. The point appears to be that if numerous people said it, it must mean something. But what, exactly, the rationale is, or whether these people are even talking about the presumed bias of the ABA (or the Senate) (or something else, such as qualifications?) is left entirely to the reader's imagination.

[78] "My hypothesis is stated repeatedly throughout the paper." Lindgren Rejoinder, *supra* note 2, at 267. He then quotes himself describing the very findings of the study. *Id.* at 267-68. And concludes by asserting: "The data clearly and obviously support the actual hypothesis of the paper." *Id.* at 269.

These are remarkable statements. They recognize no distinction between hypothesis and results. No reader of the original paper could possibly have known that what he now says was his hypothesis was anything other than the pattern of findings which he happened to wind up with in

rationale, cogent or otherwise, for deriving what he now claims was the study's hypothesis.

Professor Lindgren denounces us as fabricators by declaring that his conversation took place on the telephone while his colleague was at home, not in a hallway at the office, and was continued later in an office at the office, rather than in the hallway.[79] What is the materiality of his accusation? That is, what was it we were trying to trick readers into believing by suggesting that Professor Lindgren's conversation took place in person in a hallway at work versus on a telephone with someone at home?[80]

What matters is what he learned from it. And what of importance he learned from it he presumably reported in the article, we quoted that, and we explained why it affords no support to his theoretical claims.

Moreover, we could not have been deceiving anyone since in a footnote we provided readers the totality of what Professor Lindgren said he learned from that conversation, quoting all of it directly from his article. And to make sure the reader does not miss what he said he learned, we suggested to the reader: "That exchange is worth re-reading"[81]—because we wanted the reader to see that even on his own terms and in his own words Professor Lindgren's reasoning about the point amounted to very little support indeed. Based on the evidence we supplied, it is plain to any reader that we could not know (and did not care) whether the relevant part of the conversation literally took place over a telephone or in an office or in a hallway. The problem was not *where* the conversation took place,[82] but that Professor Lindgren offered, and still offers, nothing but an incoherent leap from "people at the White House decided to pick sitting judges" to "the ABA committee's bias will not extend to such prospective nominees."[83] We questioned

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the course of a far more generalized search for evidence of far more generalized bias.

[79] In his rejoinder, Professor Lindgren states that the conversation consisted of "nearly an hour [on the phone at home], followed by another hour interview in his office the next day." *Id.* at 279. We take him at his word. But that so much talk about a matter so important to his hypothesis/findings could yield so little substance is all the more remarkable.

[80] Professor Lindgren never says.

[81] Saks & Vidmar Critique, *supra* note 4, at 229 n.25.

[82] Or how many people said it. *See supra* note 70.

[83] The reader might wish we would spell out Professor Lindgren's chain of reasoning more fully, but we cannot because he did not (in his Study), does not (in his Rejoinder), and perhaps cannot. He tells readers of a change in White House selection strategy (nominate more judges to

both the logic of that leap and the absence of any empirical evidence bearing on it.

Professor Lindgren still offers no answer to those substantive questions. All he does is to draw the reader into an irrelevancy: whether the bit of information he received was received in a hallway or over a telephone. Professor Lindgren asserts that we have now "effectively admitted [the fabrication] by dropping it." He explains: "After I submitted an earlier draft of this rejoinder, the editors requested that they delete this factually inaccurate statement, which they did."[84] We do not know where Professor Lindgren got his inside information about the details of the editorial process which led to any of the changes in our Rejoinder as it moved from manuscript submission to publication, but we can say that what he wrote about "editors request[ing] that [we] delete this factually inaccurate statement" is itself not accurate.[85]

If we look beyond Professor Lindgren's distress that his conversation was being slighted, we find that he still has not answered the substantive criticism. He did not before—and does not now—offer any logic to tie what was learned from the conversation (wherever it took place) to the theoretical insight to which it supposedly led.

E. Et cetera

There is, of course, more: non-significant effects treated as if they were significant effects, interaction effects discussed as if they were main effects, the fact that in dichotomizing variables one always throws away information and sometimes exploits irregularities in the data, the effect

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become judges). And he says in his Study that it was consistent with the findings he had obtained and then in his Rejoinder that the Study tested and confirmed that hypothesis. What hypothesis? The implicit hypothesis seems to be that nominating more judges to become judges causes allegedly biased ABA evaluators to cease making biased evaluations. How does that work? We have no idea. As we point out in our Critique, at footnote 25 and accompanying text, there are far more plausible (less leaping) hypotheses with which the same pattern of findings is equally consistent. *See* Saks & Vidmar Critique, *supra* note 4, at 229 n.25 and accompanying text.

[84] Lindgren Rejoinder, *supra* note 2, at 279.

[85] At no time did any editor suggest to us that the phrase "hallway conversation" was factually inaccurate and therefore needed to be corrected. (Indeed, the editor recognized that it was a metaphorical characterization for which accurate and inaccurate had little if any meaning.) No useful purpose would be served by reciting further details of the discussion surrounding that particular editorial change. Perhaps it will suffice to remind readers that he who relies on hearsay runs a certain risk of being in error.

that running dozens of models has on p-levels, the alleged impropriety of criticizing a study by proposing plausible rival hypotheses, the inconsistent characterization of the data as populations or samples[86] depending on which better served a point Professor Lindgren wished to make at the moment, etc., etc. But these are smaller points and doubtless the reader is growing weary.

CONCLUSION

Professor Lindgren devoted much of his rejoinder to calling us names and trying to characterize our alleged rhetorical strategies and tactics. We will not call him names. But his rhetorical strategy has become clear, and we would like to put a name on it: Distraction and Misdirection. Professor Lindgren does not join the issue; he changes the subject. As this reply has shown, in a lengthy rejoinder Professor Lindgren was able to offer remarkably little in the way of substantive responses to our substantive criticisms. He reframed some of them into something they were not, but which he apparently thought he had a better chance of replying to; he gave weak responses;[87] he went off on barely relevant tangents; he gave partial responses; and he ignored some criticisms altogether. What he did not do was to provide substantive answers to our actual and central criticisms of his methodology, his conclusions, and his policy recommendations. Now that Professor Lindgren has had ample opportunity to respond, and has failed to do so meaningfully, our criticisms stand even more firmly than before.[88]

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[86] Professor Lindgren states: "Saks and Vidmar also state flatly that I am dealing with a population, not a sample." Lindgren Rejoinder, *supra* note 2, at 273. Actually, we stated precisely the reverse. *See* Saks & Vidmar Critique, *supra* note 4, at 247.

[87] E.g., "Lots of others do it, so why can't I?"

[88] It may bear pointing out that everything we have discussed is analysis and criticism of research methodology. On the substantive question of whether the ABA committee is politically biased or not, we are agnostic and offer no views. *See also* Saks & Vidmar Critique, *supra* note 4, at 221. We have no hypotheses of our own. At the end of the day, those who believe in the hypothesis of political bias might be entirely correct or entirely mistaken. We conclude only that the Lindgren/Federalist study makes little if any contribution to resolving the question.